The decision on this point of the future of the higher forestry training is a momentous one, since it involves no less than the future correct management of the majority of the forests of the Empire.

E. P. STEBBING.

University of Edinburgh, October 29.

British Laboratory and Scientific Glassware.

I HAVE read with much interest the letters in NATURE of November 4 from Prof. Bayliss and Mr. Frank Wood on the subject of British glassware, and I think the whole truth lies, perhaps, between the two opinions put forward. As a manufacturer of scientific apparatus, and primarily of X-ray tubes, I have had probably as trying an experience of glass as any manufacturer since 1914.

It is well known, I presume, that prior to the war the whole of the glass bulbs and tubing used in the manufacture of X-ray tubes came from Germany, and the quality was undoubtedly very fine indeed. Since 1914 we have been obliged to depend upon glass of French, American, and English manufacture. Although by no means without merit, the products of the two first-named countries were discarded directly the English makers were in a position to give us anything at all adequate to work with, and since 1916 I think we have not used any glass whatever other than that made in this country.

At the present time the position is that a glass reasonably good for our purpose is made by at least two British firms. It works well in the flame and preserves a good appearance, but it is impossible to say that it has reached the high standard set by the German product. So far as the purchasers of the finished instrument are concerned they are not affected, because the imperfections of the British glass, where they exist, manifest themselves during the manufacture of the complete X-ray tube, and the difficulties, therefore, are entirely connected with manufacture, and not with the efficiency of the working of the apparatus which is being constructed.

It has seemed to me for a long time past to be a matter for regret that the British manufacturers could not make those small final improvements which would give us exactly the material we require instead of, as at present, stopping a little short of the ideal.

I believe there is no particular difficulty at the moment in obtaining supplies of glass from Germany, but up to now I have resisted every temptation to do this, partly on general sentimental grounds, but largely because of the enormous amount of trouble which has been taken by the two firms of which I spoke in order to produce a glass suitable for X-ray purposes. I am sure that from start to finish the profit on this undertaking must have been negligible, and there have been endless experiments and a very large amount of waste, the cost of which has fallen chiefly on the glass manufacturers themselves.

For this reason I feel that every endeavour should be made to place the British glassmakers in a position whereby they could continue to produce these special glasses, the demand for which is comparatively small, but which are, nevertheless, of the very greatest importance to scientific workers in this country. Those firms engaged in my particular branch of the electromedical industry are always only too ready to cooperate in every possible way with the glass houses in order to secure in this country absolute independence in the matter of the supply of material.

CUTHBERT ANDREWS.

47 Red Lion Street, High Holborn,
London, W.C.1, November 25.

Heredity.

IN his letter to NATURE of November 25 Sir Archdall Reid has ably stated some fundamental biological truths concerning heredity, and with many of his statements I believe all biologists would agree. From the developmental point of view there is certainly a sense in which all characters are alike, arising as the result of the interplay of the germ and its environment, nature and nurture. In this limited sense it is doubtless beside the mark to inquire whether nature or nurture is more important, seeing that both are essential elements in any development at all. From this point of view it may be true, to cite Sir Archdall Reid's example, that there is no fundamental difference between the head and the scar; both may be in one sense germinal, and in another acquired.

But this does not go to the root of the matter, as may be most readily pointed out by referring to the latter part of the letter in NATURE. Sir Archdall Reid says: "The sole antecedent of non-inheritance is variation." The statement is true, of course, but he goes on to assume tacitly that all variations are in one category. Sir Archdall Reid recognises the fact, which Weismann emphasised, that "heritage travels down the germ-tract," and draws the "*necessary* [his italics] inference from this" that all characters of the individual are "innate, acquired, and inheritable in exactly the same sense and degree." But this is surely a *petitio principii*, for while all inherited characters may come to travel down the germ-tract, it does not follow that they all originated as variations in the germ-tract. It is surely legitimate to assume, until the contrary is proved, that new characters may arise (to use ordinary biological terms) as germinal variations or as impressed modifications of the soma which are not represented in the germ-tract. Indeed, this is the current distinction drawn between mutations and fluctuations. In the latter case the question will arise whether the modified soma may ultimately affect the germ-plasm; in other words, whether a modification or an acquired character may come to be inherited by bringing about an alteration in the germ-plasm. This is surely a legitimate inquiry. If so, it implies the possibility that the "scar" might ultimately, having become germinal, appear without the specific stimulus that is now necessary to call it forth.

R. RUGGLES GATES.

King's College, Strand, W.C.2.

The Mechanics of Solidity.

UNDER this title Mr. J. Innes (NATURE, November 18) suggests, for the benefit of engineers, that coefficients of thermal expansion are fairly closely related to hardness. His list of thirty-eight materials ranging from diamond to indiarubber is given in order of thermal expansion. No definition of hardness is suggested, and the figures, taken from three tables of "hardness," are admittedly somewhat conflicting.

Hardness, I take it, is due in part to closeness of atomic packing. Diamond, the hardest substance known, possesses also the lowest known atomic volume, while potassium, the softest element on Mr. Innes's list, has by far the highest atomic volume, *i.e.* has the loosest atomic packing.

Diversities in hardness depend also on how far each substance tested is removed from its melting point. Taking fourteen elements from the list, and assuming tests for hardness were made at uniform temperature, the order of degree-distance below melting point comes out:—Diamond, iridium, platinum, iron, gold, copper silver, aluminium, arsenic, antimony, lead, tin, bismuth, and sulphur.

© 1920 Nature Publishing Group

The order of hardness is:—Diamond, iridium, platinum, iron, arsenic, antimony, gold-copper-silver, aluminium, tin, bismuth, and lead-sulphur. (It is probable that arsenic and antimony were tested in a crystalline state, in that respect differing from the three metals above and the four below them.)

The order of ascending coefficients of expansion is:—Diamond, arsenic, iridium, platinum, antimony, iron, bismuth, gold, copper, silver, tin, aluminium, lead, and sulphur. Here the agreement is not so close as before.

Doubtless coefficients of expansion themselves depend partly on how far the mean range of temperature from which they are calculated is removed from the melting point. For strict comparison, what are known as "corresponding temperatures" should be taken. If this is done for coefficients of expansion of gases, then the values become identical; with solids (or liquids) the coefficients would approach, but never reach, uniformity.

In practice engineers are bound to consider arbitrary temperatures which will affect their mixed materials, but a knowledge derived from comparison of physical properties at corresponding temperatures would enable them to predict special changes among the mixed material which would occur when temperatures rise or fall.

REGINALD G. DURRANT.

University College, Reading.

The Hardening of Metals under Mechanical Treatment.

IT appears from an article by Mr. Ernest A. Smith in NATURE of November 18, p. 381, that the cause of the hardening of metals under mechanical treatment is still regarded as obscure.

May I suggest, for the consideration of research associations and others concerned, that all the phenomena of plastic deformation, including hardening by distortion, are aspects of what Osborne Reynolds has called "dilatancy," and that this twentieth century is no time for random empirical experiments conducted without either guiding principles or clearly defined objectives?

Reynolds has shown that the density of a granular solid must change when the solid is distorted. There is ample evidence that distortion alters the density of metals, and no very valid reason for assigning the accompanying alteration of "hardness" (and other properties) to anything but a change in the pattern of the grains, *i.e.* to dilatancy.

Systematic experimental investigation can scarcely fail to have far-reaching results, and may even bridge the gulf between mechanics and the more exact sciences.

J. INNES.

12 Edward's Road, Whitley Bay,
Northumberland, November 22.

Tube-dwelling Phase in the Development of the Lobster.

WHILST the development of the lobster from the Schizopod stage onward to an inch or a little more is fairly well known, the great rarity of the stages between that and 3 in. or 4 in. (second year) has often puzzled marine zoologists. For instance, only once in many years has a small lobster of about 4 in. been seen at St. Andrews, and this example was tossed amidst a vast quantity of débris on shore after a violent storm. Prof. Prince, Dominion Commissioner of Fisheries for Canada, and president-elect of the American Fisheries Society, who has inaugurated many important advances in scientific fisheries work, tells me that Prof. Knight, who has been investigating the subject, finds that "after the pelagic stage the young lobster appears to frequent shallow bays and make a definite burrow with two entrances, and it sits on guard at one, but if in danger escapes by the other. It is very quick in emerging, but Prof. Knight and his assistant got 200 to 300 in a bay in Prince Edward Island. Now we know the reason of our failing to capture these very small lobsters from $1\frac{1}{2}$ in. to 3 in. long. The dredge cannot secure them, yet they must occur in countless millions in our Canadian bays, since many more than 100,000,000 adults are taken in our waters for canning and the live-lobster trade in good years." Thus the rarity of the little lobsters of the stage indicated is explained.

It is interesting that the adult, as shown by Dr. H. C. Williamson, of the Scottish Fishery Board's staff, has a similar fondness for cavities, which it searches for with its antennæ, and will even turn out a weaker neighbour and seize its shelter.

W. C. MCINTOSH.

Contractile Vacuoles.

I HAVE just read W. Stempel's paper, "Ueber die Funktion der pulsierenden Vacuole," to which Prof. Bayliss kindly directed my attention in NATURE of November 18, p. 376. Stempel's idea as to how the contractile vacuole works appears to differ fundamentally from my conception of its mechanism. He regards it as a preformed organ of the cell, developed to eliminate the waste products of metabolism, these products being introduced into the vacuole by the radiating canals which he endows with peristaltic action. He further postulates the existence of non-return valves between these radiating canals and the vacuole, and also of one at the point of exit of the fluid to the exterior. He considers that the evacuation of the contents is effected by the osmotic pressure in the vacuole, opening the valve and thrusting out the fluid, by the surface tension of the extruded drop, and by the pressure of the protoplasm. He does not indicate that the elasticity of the protoplasm or its tenacity enters into the mechanism. My suggestion may be summed up in the much simpler statement that the contractile vacuole is a necessary development in the protoplasmic semi-permeable gel wherever sufficient soluble material accumulates, the radiating canals being formed by the elastic recovery of the gel after rupture.

HENRY H. DIXON.

School of Botany, Trinity College, Dublin,
November 20.

Leptocephalus of Conger in the Firth of Clyde.

IN a recent issue (vol. xii., No. 2, July, 1920) of the Journ. Mar. Biol. Assoc. Mr. E. Ford, in a "Note on a Leptocephalus Stage of the Conger," gives a "summary of captures around the British Isles." To the records there given, may I add two from the Firth of Clyde?

(1) July, 1907; off Keppel Pier; 3 fms.; length, 120 mm.; depth, excluding fins, 8 mm.; a distinct row of black spots along the lateral line. Taken from the stomach of a saithe (*Gadus virens*).

(2) March, 1908; Ardneil Bay, 10 fms.; taken from the stomach of a cod (*G. callarius*); too much digested to give any details.

RICHARD ELMHIRST.

Marine Biological Station, Keppel, Millport.

Spiranthes autumnalis.

SIR HERBERT MAXWELL will, I am sure, allow me to make a little correction in his statement on p. 409 of NATURE of November 25; it was not Sir Joseph Hooker, but Sir William who in 1843 doubtfully described a solitary specimen of ***Veronica tetragona*** as a species of the coniferous genus **Podocarpus**. Oddly enough, the plant was correctly described from a flowering specimen in the same volume of Hooker's "Icones" on a later plate.

B. DAYDON JACKSON.

Linnean Society, Burlington House,
London, W.1, November 26.

© 1920 Nature Publishing Group